

**IMPROVING WATER RESOURCES  
DECISION-MAKING: FISHERIES MODULE  
INTEGRATION WITH DWR'S WATER SYSTEM  
OPERATION SIMULATION MODELS**

**Andrew J Fecko**

## **Public Comments**

No public comments were received for this proposal.

# Technical Synthesis Panel Review

## Proposal Title

#0288: IMPROVING WATER RESOURCES DECISION-MAKING: FISHERIES  
MODULE INTEGRATION WITH DWR'S WATER SYSTEM OPERATION  
SIMULATION MODELS

Final Panel Rating
inadequate

## Technical Synthesis Panel (Primary) Review

### TSP Primary Reviewer's Evaluation Summary And Rating:

In general, all of the technical reviewers were quite critical of this proposal for several primary reasons, including (1) the proposed work appears to be part of the basic mission of the agencies that are involved which brings into question the reasonableness of requesting outside funding for the work, (2) the proposed work is not based on scientifically testable hypotheses, but rather involves integration of existing technology into a more comprehensive model, (3) the budget seems excessive for the proposed outcome which is essentially a pilot project for integrating a single-species fish module into the existing operational models. In spite of these criticisms, one of the reviewers acknowledged that the outcome would provide a useful tool for water managers.

### Additional Comments:

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#0288: IMPROVING WATER RESOURCES DECISION-MAKING: FISHERIES MODULE INTEGRATIO...

## Technical Synthesis Panel Review

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## Technical Synthesis Panel (Discussion) Review

### TSP Observations, Findings And Recommendations:

The applicants seek funding to integrate a single-species model into an operations model. There are no hypotheses developed. This project is largely a software-development activity. If developed, this model would be a useful tool, however, the panel was concerned that, even if the models were successfully integrated, the result would not be transferable to other species in the system. The budget seemed extraordinarily large for the work proposed and for the anticipated benefits. The external reviewers wrote lengthy and critical comments and the panel agreed with them and easily reached a consensus that the proposal was deficient in many areas.

# Technical Review #1

proposal title: IMPROVING WATER RESOURCES DECISION-MAKING: FISHERIES  
MODULE INTEGRATION WITH DWR'S WATER SYSTEM OPERATION  
SIMULATION MODELS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals and objectives of this proposal (i.e., link a variety of biological models to existing water models) are reasonable and appropriate although there are no explicit or scientifically meaningful hypotheses described in the proposal. In addition, I immediately wondered why this work doesn't fall under the scope of work of the agencies involved? It's certainly something I would never expect to see funded in another national competitive grants program such as Sea Grant, USDA or NSF. This may seem like a harsh statement but there are serious flaws with the proposal as described below. I certainly question whether outside funding agencies should be funding existing state agencies to meet and identify personnel with common interests/responsibilities, "refinement and clarification of project goals", etc. I mean, what are these folks doing submitting a proposal when they don't even have their goals clearly stated and refined and have to seek outside funds to performs such tasks. The whole of Phase 1 should have been completed prior to the</p>
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## Technical Review #1

	submission of the proposal.
Rating	poor

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	There really isn't much in the proposal regarding a scientifically meaningful conceptual model, although I recognize that these agencies do need a tool like they describe. The conceptual models presented (Figs. 1-2) are just very general box and arrow diagrams that show what will go into the fisheries module and how the fisheries module will be linked to other models such as DSM and CalSim. There is no description of the dynamics of the Bay-Delta system or anything that shows that the PI's understand the conceptual underpinnings of the system. There is nothing tested or falsifiable in the justification nor are we given any specifics about how it will fit within an adaptive management framework. These points are essential for any thorough reviewer to give a positive review of the proposal. Finally, the proposal is not well justified with respect to the current scientific knowledge of the Bay-Delta system. In fact, there isn't a single peer-reviewed, scientific paper listed in the literature cited. There simply is no evidence in the proposal that this work will substantively advance our scientific understanding of the Bay-Delta ecosystem.
Rating	poor

### Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be

# Technical Review #1

useful to decision makers?

Comments	<p>For a proposal to an outside funding organization, the approach is not well designed, nor am I able to tell if it's appropriate for the task at hand. This project proposes linking a fisheries module to existing models to derive a more complete and realistic model of the Bay-Delta ecosystem. The specific fisheries model is not described in detail (I'm not really sure that it's described at all!) in the proposal. Will it be an age-structured model, a non-age structured model, and Individual Based Model, will it be based on difference or differential equations? These are all very basic questions, but it looks to me like this proposal was written by engineers who have little knowledge of fisheries modelling. There are several models listed in an appendix but the models listed for stream fishes are not particularly recent nor do they include important biological components (i.e., effects of prey availability) which has been shown to be extremely important to habitat selection in stream fishes including salmonids (see Kurt Fausch's early work, work by Nick Hughes, Larry Dill, Tamara Grand and Jennifer Hill). I will admit that the listed models (IFIM, Phabsim) are widely used by management agencies but they have had little validation especially with respect to their biological realism over time. In addition, there is a complete lack of citations to peer reviewed papers that deal with many of these models, which shows a lack of understanding of the current knowledge base. In fact there are published fish population models that integrate flow data with population dynamics models, several of which were developed for California systems (see Yetta Jaeger and Web. Van Winkle's papers on California stream salmonids) -- are these papers cited -- no name. I could go on, after all there's no detail about the model nor how it will be calibrated or tested. Very perplexing for a proposal that is going to build a model. I have little faith that any new novel information that will stand up to scientific scrutiny</p>
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## Technical Review #1

	will be provided by the PI's
Rating	poor

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?  
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Given the comments above and after perusing the qualifications of the authors I think there is little chance that this proposal will produce new and meaningful scientific or management-oriented information.
Rating	poor

## Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	No monitoring, but there should be. That is the model needs to be validated and its predictions compared to actual fish population sizes, escapement levels, recruitment, etc. What's the point of building a model when you don't calibrate and test it?
Rating	poor

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	An integrated, multi-level model for the Bay-Delta system would be very useful if it was properly constructed and validated over time.
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## Technical Review #1

Rating	not applicable
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### Additional Comments

Comments	We ran into quite a few proposals like this on the CalFed Monitoring Technical Panel. These folks just don't seem to understand what goes into writing a proposal that can pass the scrutiny of outside reviewers that are active researchers.
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### Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The goal of this proposal is to develop and integrate a fisheries model (probably several given the diversity of species they want to address -- from cyprinids to salmonids and sturgeon) yet based on the information submitted, it appears that not one fisheries biologist was involved in writing the proposal. The fisheries folks are listed as TBD which I assume means "To Be Determined". How can any reviewer have confidence that this proposal will produce meaningful results when the most important folks (i.e., fisheries biologists and modelers) are not even on the proposal yet. In addition, the engineers have few or no publications in peer-reviewed scientific journals (see the C.V.'s in the appendices) which leads me to suspect that the work produced will not be able to pass the scrutiny of peer-review. Such scrutiny is essential for any meaningful scientific or management-oriented piece of research.
Rating	poor

## Technical Review #1

### Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This budget is excessive, over \$900K. Frankly this project isn't close to being ready for funding
Rating	poor

### Overall

Provide a brief explanation of your summary rating.

Comments	Frankly, the sections above provide a large amount of information regarding why this proposal should not be funded. There are crucial shortcomings in virtually every aspect of the proposal except the justification (yes they do need a model like this). Specific shortcomings include a lack of appropriate: 1) methodologies (not described at all!), 2) hypotheses, 3) evaluation procedures (none) and 4) qualified personnel (no named fisheries biologists or modelers as PI's).
Rating	poor

# Technical Review #2

proposal title: IMPROVING WATER RESOURCES DECISION-MAKING: FISHERIES  
MODULE INTEGRATION WITH DWR'S WATER SYSTEM OPERATION  
SIMULATION MODELS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>ARE THE GOALS, OBJECTIVES AND HYPOTHESES CLEARLY STATED AND INTERNALLY CONSISTENT? Yes. The applicants clearly state the objectives and are internally consistent throughout the proposal.</p> <p>IS THE IDEA TIMELY AND IMPORTANT? The idea itself is simply to link a fish habitat model to DWR's (the applicant) own physical and economic models. This is an important and topical idea, but by no means original. There is little acknowledgment in the application of other previous and ongoing efforts to do just what they are seeking to do (e.g. Harmoni-CA [<a href="http://www.harmoni-ca.info/">http://www.harmoni-ca.info/</a>] and Harmoni-IT [<a href="http://www.harmonit.org/">http://www.harmonit.org/</a>]). The applicants raise the problem of modelling fisheries habitat for multiple species and life-stages simultaneously. This is an important issue and something the ecohydraulics community and literature struggle with continuously. However, I am sceptical that the review and brainstorming exercises that the applicants are proposing would produce any real substance towards tackling this problem. A better place to start would be by simply reading this literature and doing some habitat simulations for a small sub number of species (say 5-10). The applicants instead propose to just model one species, which is what most people in this field already do with ease and other CALFED funded</p>
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	projects have already done successfully [e.g. Wheaton, et al., 2004a; Wheaton, et al., 2004b].
<b>Rating</b>	poor

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

<b>Comments</b>	<p><b>IS THE STUDY JUSTIFIED RELATIVE TO EXISTING KNOWLEDGE?</b></p> <p>The study may be justified within the context of DWR's own internal goals for linking their models to a fish habitat model. However, relative to existing knowledge the study is totally unjustified. What the applicants are proposing to do is essentially the equivalent of what a masters or doctoral student would due in their literature review phase (at a likely maximum cost of roughly \$20,000 to \$40,000). They claim that a multi-species fisheries module would be too ambitious to develop within the study period. Considering that such fisheries modules already exist, this is completely ill-founded. The overarching theme of the proposal seems a desire to consolidate many of the State of California's modelling efforts and data into one standardized system. Such an effort may make it easier to make consistent comparisons across a region. However, this is not a scientific endeavour and there are serious sacrifices and information loss that must be accepted to achieve such conformity. If there is anything that is emerging from the study of complex natural systems like the Bay-Delta, it is that spatial and historical contingencies matter [e.g. Phillips, 2001b; Phillips, 2001a]. Thus, one model-structure over an incredibly diverse region may be inappropriate. Furthermore, a plurality of parallel model approaches is more conducive to highlighting unforeseen problems and issues.</p>
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	<p>IS A CONCEPTUAL MODEL CLEARLY STATED IN THE PROPOSAL AND DOES IT EXPLAIN THE UNDERLYING BASIS FOR THE PROPOSED WORK? The applicants do outline a clear conceptual model, which is the underlying basis for the review they are proposing. Figures 1 and 2 clearly and reasonably articulate the applicant's vision of how the project would proceed and how a fisheries module would link with other existing models and data sets. Neither of these conceptual models represent new ideas. These are simply a clear and transparent diagrammatic mapping of what is basically common knowledge in ecohydraulics [e.g. Bockelmann, et al., 2004; Poff, 2004].</p> <p>IS THE SELECTION OF RESEARCH, PILOT OR DEMONSTRATION PROJECT, OR A FULL-SCALE IMPLEMENTATION PROJECT JUSTIFIED? No. This application is considered a pilot or scoping project, but I contend it is nothing more than a very expensive literature review and a training session on how to run some ecohydraulic models (e.g PHABSIM) that already exist and are used in practice. For the proposed budget, a demonstration project or full-scale implementation project would be justified. Certainly on the basis of what the ecohydraulic community can already do, this would be feasible.</p>
Rating	poor

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>IS THE APPROACH WELL DESIGNED AND APPROPRIATE FOR MEETING THE OBJECTIVES OF THE PROJECT? The approach is generally well designed insofar as a thorough review and scoping study is concerned. Furthermore, there is little doubt that the approach would lead to fulfilling their objectives. The problem is not the</p>
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applicant's approach, but that the objectives themselves are set too low.

If one is to critique their approach more specifically, I do have specific concerns about the logistical organization. Many of the more substantial tasks (e.g. tasks 7-9) are divided up between roughly 20 people on the project team. Each individual is basically allotted 3-4 days to complete the task. In my experience on larger projects such as this, it is better to have a few people produce a lot, than a lot of people produce nothing. Although I can understand that given the multi-agency effort and variety of models and people involved, such a structure might be proposed. However, I have serious concerns as to its efficiency. Arguably, the most substantial piece of the proposed review is the development of the test case study and associated software. The applicants have failed to find this expertise from within their 20-strong team or identify a specific person(s) to carry out this work. Instead, an unidentified subcontractor is identified to do this work. Surely, for a proposal of this magnitude, a qualified team could have been constructed for the application?

IS THE APPROACH FEASIBLE? Yes. Not only is the approach feasible, it has been done by others already (e.g. Harmoni-IT). I have no doubt they could find an academic or sub-consultant to accomplish their stated objectives.

ARE RESULTS LIKELY TO ADD TO THE BASE OF KNOWLEDGE? Whose base of knowledge? Surely, the results would help DWR, USGS, USBR, CDFG, USFWS and NOAA staff expand their knowledge base. The results would probably even be helpful to CALFED as a whole. However, there are much more efficient ways to achieve this same knowledge (e.g. read the existing literature). I see very little in this proposal that would be considered an original scientific contribution or worthy of publication on the grounds

of originality in a peer-review journal.

IS THE PROJECT LIKELY TO GENERATE NOVEL INFORMATION, METHODOLOGY, OR APPROACHES? (See previous). No. In all facets of environmental modelling there are similar efforts to link models of different types [e.g. Van Asselt and Rotmans, 2002; Lempert, et al., 2003]. This project simply seeks to do the same with the applicants' own models. If that is all the applicants really want to do, a potential solution already exists: 1. Pick an off the shelf fish habitat model (e.g. PHABSIM, CASiMiR, etc.). Most of these models are already geared to run directly off the hydrodynamic outputs of models like CALSIM (e.g. velocities, depths, substrates). Models like CASiMiR already have modules for exploring economic alternatives of hydropower generation management scenarios (<http://www.sjeweb.de/publikationenEN.html>). 2. Choose the species and lifestages you want to model and acquire habitat suitability or preference curves. 3. Use the OPEN-MI framework to link all the models together (<http://www.harmonit.org/>). The OPEN-MI framework is a graphical interface that links any models or databases that are time-step based together. It provides development tools for linking models of differing spatial scales, spatial dimensions (1D, 2D or 3D) and temporal resolutions. The most common example is to use a hydrologic rainfall-runoff model's Q output to drive the boundary conditions for a hydrodynamic reach model, which in turn drives a fish habitat model. The framework is coded in C# in the .net framework, but does not require that models are recoded in this language. Models from most languages (FORTRAN, C, C+, etc.) can be made OPEN-MI compliant without major recoding. The user interfaces for these models remain the same. Nothing that the applicants are proposing strikes me as novel.

WILL THE INFORMATION ULTIMATELY BE USEFUL TO DECISION MAKERS? Despite all my criticisms, the information produced by this project probably would ultimately be

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	<p>useful to decision makers. The most noteworthy contributions are the linking and improved accessibility of databases. However, I have two major caveats. First, is it worth the price tag? Second, is it better to give decision makers real individualized advice or consistent simplified advice that has a greater potential to be wrong? That is, the applicants claim that by transparently identifying assumptions behind their standardized modelling techniques (p. 8) that this will reduce scepticism of models. I fail to see how this is useful to decision makers. In my experience, decision makers could care less which version of the Navier-Stokes equations you used in your hydrodynamic model. The applicants are right to consider the uncertainties in their models, but the burden lies on them as scientists to communicate this information to decision makers in a way that is understandable and useful.</p>
Rating	poor

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?  
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>IS THE APPROACH FULLY DOCUMENTED AND TECHNICALLY FEASIBLE? What is there to document? The applicants fully cite all the exiting models that they want to link to the proposed fish model. Unfortunately, there is no documentation or citation of the plethora of existing fish models, which they might choose to use. The approach is completely technically feasible because it has already been done (again see Harmoni Projects). Although it is important to have localized solutions to localized problems, it is unfortunate that the scientific restoration community continues to ignore the 'lessons learnt' by others. During one of my own</p>
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recent literature reviews, I was shocked by the number of projects taking place concurrently throughout the world to achieve what are essentially the same objectives on the grounds that no one else has done it. This proposal, is unfortunately, another example of a good intentioned project that grossly neglects what the scientific community should already know (should because it is published).

WHAT IS THE LIKELIHOOD OF SUCCESS? If success means to spend a million dollars, have some meetings and publish a literature review and plan, the likelihood of success is high. If success means how this project would be judged in scientific peer-review, the likelihood of success is low. If success means how this project would fare when put under the microscope by taxpayer-watchdog groups concerned about bureaucracy, this project would be a failure.

IS THE SCALE OF THE PROJECT CONSISTENT WITH THE OBJECTIVES AND WITHIN THE GRASP OF AUTHORS? The scale of the project is inconsistent with the objectives. The objectives are similar to those typically achieved within the earlier phases of any scientific study. Yet the applicants go to great lengths to turn what is essentially a scoping study into a major project. The objectives the authors propose seem well within their grasp. However, if the objectives were more appropriately identified as their long-term goal of producing a working fisheries module, it seems they are outside the grasp of the authors. This is supported by their apparent lack of knowledge and understanding of existing fisheries models, the literature, and proposal to

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	subcontract out the meat of the proposal to a as-of-yet to be identified subcontractor.
Rating	fair

## Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	IF APPLICABLE, IS MONITORING APPROPRIATELY DESIGNED (PRE-POST COMPARISONS; TREATMENT-CONTROL COMPARISONS)? N.A. ARE THERE PLANS TO INTERPRET MONITORING DATA OR OTHERWISE DEVELOP INFORMATION? N.A.
Rating	not applicable

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	ARE PRODUCTS OF VALUE LIKELY FROM THE PROJECT? This project is sold not on what it will produce but what it will allow the production of at some point beyond its three year life span. What it will produce is a comprehensive review and feasibility study. This product will no doubt be of some interest to those doing similar studies. However, I don't see the specific products of this study as that valuable to the larger CALFED community or the scientific community. Particularly when you consider similar studies already exist [e.g. Hutchings, et al., 2002]. ARE CONTRIBUTIONS TO LARGER DATA MANAGEMENT SYSTEMS RELEVANT AND CONSIDERED? The applicants have done an excellent job of inventorying larger available data bases within the CALFED agencies and California (Appendix 1). The further refinement and linking of these data bases seems to be one of the most important
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	<p>products of the project.</p> <p>ARE INTERPRETIVE (OR INTERPRETABLE) OUTCOMES LIKELY FROM THE PROJECT? The aim of the project seems to be to produce a single outcome: a plan and procedure for implementing a fisheries module. This seems predicated on the notion that a thorough review and planning process will reveal a single best way forward. This is naïve at best. Such a review will certainly provide interpretations about what would be a reasonable way or ways forward. Is this a product that will be of great benefit to anyone outside the partner-institutions of the applicants or the overall CALFED community or larger scientific community? I would venture to say, probably not. So, to the extent that a plan is interpretable, the answer to the question might be yes.</p>
Rating	poor

## Additional Comments

Comments	<p>Why would stakeholders or the public want to participate in the development of a bunch of technical models? Stakeholders and the public may be interested in the results of such models or in influencing what scientific questions are asked, or how that information is used to make decisions.</p> <p>The following quote is telling of the premise behind this application: 'The capabilities, as well as the limitations, that will be identified by completing this project will help reduce skepticism about dubious assumptions or improper applications of the models. The assumptions and limitations of the models used will be explicitly stated.' In other words, we don't know what the limitations are, but we'll spend a lot of money to find out just so no one will doubt our models.</p>
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## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	<p>I am not familiar specifically with any of the authors' work. This should not be held against them, but it should be noted that my comments on their capabilities are therefore only based on reviewing their resumes and this proposal and not from having read their previous publications.</p> <p>WHAT IS THE TRACK RECORD OF AUTHORS IN TERMS OF PAST PERFORMANCE? Most of the authors' resumes suggest that they have a strong track record of carrying out their respective jobs. Some of the individual authors have reasonable peer-reviewed publication records, whereas other authors seem more heavily weighted with management experience (of little importance to achieving this proposal's objectives in my opinion). The project manager appears to have successfully managed a \$2 to \$3 million dollar Proposition 50 contract. I know nothing about the details of this project. It is difficult for me to comment any further on their track records on the basis of information provided. Regardless of the authors' past track records in their respective jobs, there is very little in this proposal to indicate a track record or experience with the type of work they are proposing. This is not to say that they are incapable, but it is concerning.</p> <p>IS THE PROJECT TEAM QUALIFIED TO EFFICIENTLY AND EFFECTIVELY IMPLEMENT THE PROPOSED PROJECT? There is nothing in the application to suggest that this team is not qualified to effectively implement the project objectives</p>
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	<p>as proposed. In my opinion, a project of this scope to meet their stated objectives is by definition inefficient. I am also concerned with the lack of expertise within the project team with respect to ecohydraulics, fisheries, ecology and geomorphology. The team seems well qualified and competent as engineers and hydrologists. However, their proposal fundamentally seeks to develop a fish habitat module. From a quick review of their resumes and publication records, there does not appear to be a single expert on the team in these areas. If there is such expertise within the team, it was not adequately emphasized in the proposal.</p> <p>DO THEY HAVE AVAILABLE THE INFRASTRUCTURE AND OTHER ASPECTS OF SUPPORT NECESSARY TO ACCOMPLISH THE PROJECT? Anyone with web-access, journal subscriptions and access to an academic library has adequate infrastructure to complete this project as it is primarily a review. I am not familiar with whether or not the respective agencies of the applicants access to these sources. Particularly if the applicants do not have subscriptions or access to the latest peer-reviewed journals, this project could be at risk. Of critical importance to this project is that the most recent developments in the ecohydraulics field are reviewed. It is clear from the application that they have not already done this. If the sub consultant slated to do much of the work happens to be a consultant, it is unlikely they would have access either.</p>
Rating	fair

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### Budget

Is the budget reasonable and adequate for the work proposed?

Comments	<p>No. To spend roughly \$1 million on a glorified conceptual model and research plan that could easily be developed through a more rigorous literature review seems to me a waste of money. What is worse is that everything they are proposing has already been done (with different models by different investigators) and shows a lack of awareness of the peer-reviewed literature. In fact the bibliography cites 15 sources, only one of which is from a peer-reviewed journal and the vast majority of which are grey literature published by the applicants or their co-workers. Thus, CALFED's Science Program needs to decide whether it wishes to fund original science, or a collection of agency wish-list items. This is not meant to discourage the efforts of these agencies to achieve this project's goals. I strongly believe if they were to contact the right researchers, the entire scope of this project could be completed for less than \$50,000 in under a year (e.g. fund a graduate student). As proposed, this is poor and inefficient spending of CALFED funds.</p>
Rating	poor

### Overall

Provide a brief explanation of your summary rating.

Comments	<p>PROVIDE A BRIEF EXPLANATION OF YOUR SUMMARY RATING. I fundamentally disagree with the the basic premises of this proposal. The first is that what they want (a multi-species fisheries module) is too difficult to produce yet: "The complexity of the biology and ecology of fisheries resources prevents a three-year duration project from accomplishing full development of a functional fisheries module for all sensitive</p>
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species without an exorbitant budget and effort. However, a plan to develop a fully functional module for a selected species is achievable.." Perhaps the project team feels they don't have the expertise to implement this with their resources. This does not mean that it couldn't be achieved for a similar amount of money. The second premise is essentially that standardized simulation models are what are best for decision making. This is a dangerous line of logic. I agree that a standardized simulation output might make decision making easier, but that does NOT insure that the decisions are any better. I would have strong reservations about putting all eggs in one basket just for the sake of conformity. There is something valuable in having a plurality of approaches and views on a such multi-faceted questions. If for example the applicants can produce the most brilliant fisheries module in the world, the results may be entirely wrong if one of the other built in components (e.g. hydrodynamic model) turns out to be flawed.

Furthermore, there is nothing scientifically original in this proposal. The proposal is filled with all the correct jargon and buzzwords, but fails to propose any significant way to work with them. For example, the title suggests that this project will 'improve water resources decision making.' Decision support systems are becoming more ubiquitous and are quite an interesting line of research [e.g. Petterman and Peters, 1998; Jensen, et al., 2000; Zsuffa, 2000; Clark, 2002; Clark and Richards, 2002; Moschandreas and Karuchit, 2002; Adriaenssens, et al., 2003; ISAB, 2003]... this is one of the reasons I chose to accept this review. I do think that the project objectives are worth pursuing by the applicants regardless of the funding of this proposal. I have provided the few cited references I made below:

### References:

Adriaenssens, V., Baets, B. D., Goethals, P. L. M. and

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	<p>B.C., 89-104 pp., 1998. Phillips, J. D., Contingency and generalization in pedology, as exemplified by texture-contrast soils, <i>Geoderma</i>, 102(3-4), 347-370, 2001a. Phillips, J. D., Human impacts on the environment: Unpredictability and the primacy of place, <i>Physical Geography</i>, 22, 321-332, 2001b. Poff, N. L., Natural Flow Regime as Paradigm for River Restoration - a Hydroecological Context for Ecohydraulics?, in <i>Fifth International Symposium on Ecohydraulics: Aquatic Habitats: Analysis and Restoration</i>, vol. 1, edited by Garcia, D. and Martinez, P. V., IAHR-AIRH, Madrid, Spain, 2004. Van Asselt, M. B. A. and Rotmans, J., Uncertainty in integrated assessment modelling - From positivism to pluralism, <i>Climatic Change</i>, 54(1-2), 75-105, 2002. Wheaton, J. M., Pasternack, G. B. and Merz, J. E., Spawning Habitat Rehabilitation - II. Using Hypothesis Testing and Development in Design, Mokelumne River, California, U.S.A., <i>International Journal of River Basin Management</i>, 2, 21-37, 2004a. Wheaton, J. M., Pasternack, G. B. and Merz, J. E., Spawning Habitat Rehabilitation - I. Conceptual Approach and Methods, <i>International Journal of River Basin Management</i>, 2, 3-20, 2004b. Zsuffa, I. J., Multi-criteria decision support for revitalization of river floodplains, in, <i>Wageningen University, The Netherlands</i>, 155 pp. 2000.</p>
Rating	poor

# Technical Review #3

proposal title: IMPROVING WATER RESOURCES DECISION-MAKING: FISHERIES  
MODULE INTEGRATION WITH DWR'S WATER SYSTEM OPERATION  
SIMULATION MODELS

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The purpose of this project was clearly stated as to develop a fisheries module linking hydrology models with economic modules. It is proposed that a prototype fisheries module be based on a single (yet to be determined) sensitive fish species, and later efforts could expand to include multiple species. Output from this module would input to an existing economics module, output from which would then feed back into the water flow and quality models. I find this potentially inconsistent and flawed. It was not made clear, or even addressed from my reading of the proposal, how a sensitive species that is neither recreational or commercial important would effectively inform the economics module. Is conservation of sensitive species the objective in assessing the fishery objectives, or is it optimization of economic returns, or is it both? It would seem necessary to feed the fisheries module into a regulatory module, which evaluates alternate management strategies and options for water management, then out put from this latter module could usefully inform an economics module. It also seems limited to run all feedback through the economics module, and it seems important to have feedback to water management models that is independent of economic constraints, or at least have tradeoffs evaluated not only within an economic framework.</p>
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<b>Rating</b>	good
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## Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

<b>Comments</b>	The study is certainly justified and the proposal makes a good case for its need and basis. Implementation of a fisheries module appears to be a long standing need within DWR's modelling framework, and this proposal for what is essentially a pilot project would provide a necessary framework for future elaboration. This is a commendable enterprise that could be of considerable value for management of the state's water resources. However, one may wonder if this project is not a primary mission of DWR or CFG and that it should be internally funded.
<b>Rating</b>	excellent

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

<b>Comments</b>	The approach appears to be one of design by committee and it might be argued that it might be more effective approach via a well supported core team. Each task is set by a large number (dozen or more) people from various agencies and locations that are supposed to complete the work in the period of a few days each. This seems a very dispersed approach to a problem with very specific needs. A core team working through a staged series of workshops might be a better approach to develop the models and software, and a staged
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	<p>series of workshops or focused collaborations might just as well bring in the breadth of experience needed.</p> <p>The modelling effort is implicitly a single species approach, and it is proposed to work from one or more indicator species. This can seriously limit the scope of the project, as species such as delta smelt, Chinook salmon, and steelhead trout all have different life history requirements and critical habitat/flow requirements that are partially or wholly isolated in time and space. An approach that was constructed within a whole basin context for a suite of species meaningful to DWR constraints would produce a more dynamic, realistic, and useful framework. Perhaps this is a goal of this project, but if so it is unstated, and it is not clear how building isolated modules for a succession of sensitive species will achieve an integrated picture of the system.</p>
<b>Rating</b>	<b>fair</b>

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?  
Is the scale of the project consistent with the objectives and within the grasp of authors?

<b>Comments</b>	<p>Even though there is room for criticism regarding the approach taken (as above), as given, it is sufficiently documented to warrant confidence that it can be successfully accomplished. It will draw upon a variety of existing models for fish populations to habitat and stream flows and it is unlikely that a successful fishery module could not be incorporated into the existing DWR modelling framework. This is an attainable project with generous timelines.</p>
<b>Rating</b>	<b>excellent</b>

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### Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

<b>Comments</b>	
<b>Rating</b>	not applicable

### Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

<b>Comments</b>	The products delivered from this project are potentially of high value, and it has the capacity to provide a useful synthesis of extant data sets. This project will enhance a heavily used operational model for water management in the state by incorporating fishery variables, thus it will have broad implications for a wide array of important management decisions.
<b>Rating</b>	very good

### Additional Comments

<b>Comments</b>
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### Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

<b>Comments</b>	Staff, both primary and secondary, appear well qualified to carry out and complete this work; some have distinguished records in developing and maintaining model and software for both water resource (DWR) and
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### Technical Review #3

	fishery/hydrology models (USGS), and others are capable field biologists with a practical grasp of the issues and contexts involved.
Rating	excellent

## Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems exorbitant for the proposed task. The approach taken to combine the efforts of many people makes this a costly project, and potentially is of diluted benefit when compared to a core-group approach.
Rating	good

## Overall

Provide a brief explanation of your summary rating.

Comments	While this proposal has sound objectives and supporting capabilities, I have serious doubts about its objectives and approach. Developing a fisheries module that only informs an economic module, with no other feedback to the water resource model is a red flag from a biologists point of view. It is not clear how this will provide realistic management scenarios for both conservation and economic problems. The single species approach appears limited, and it is not clear how a whole basin perspective will be gained. It seems that money might be better spent on a more focused effort with a core development group. External expertise could still be obtained via focused consultations, collaborations, and workshops, which might also result in savings. It is also worth asking if this work is not a primary responsibility of DWR and USFWS, and that there could be cost reduction from agency contributions.
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<b>Rating</b>	<b>good</b>
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